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GEOLOGY.—*Major causes of land and sea oscillations.*[1] E. O. ULRICH, U. S. Geological Survey.

That the position of the strandline—hence the relation of land and sea levels—is and has ever been subject to change is a fact now established beyond all possible contradiction. The evidence shows that at times the shoreline retreated, leaving such features as elevated seaplains and cliffs on the enlarged land areas; at other times the seas advanced on the land, drowning previous river valleys, cutting new seaplains, and laying marine deposits much farther inland than before. These frequently recurring positive and negative movements of the strandline varied greatly in amount, but on the whole they were rhythmic in occurrence and volume. But neither the record of these movements nor the rhythm that runs through it is at all simple. Most of the criteria by which we determine that submergence has occurred in one case and emergence in another are relatively simple and easily applied. But when it comes to correlating the successive stages of emergence and submergence in different localities, or when we seek to arrange the movements in proper sequence and to determine their relative duration, the problems become involved and often exceedingly complex.

The evidence presented, especially in the past few years, by Vaughan, Daly, and Barrell seems to prove that at least the

[1] Presidential address delivered before the Geological Society of Washington, Dec. 10, 1919.

marginal parts of the continents have been subjected repeatedly in recent geologic ages to positive and negative displacements of the strandline; also that the vertical element of these oscillations is not uniform in amount at different places. Considering only the Pleistocene to Recent movements, their differential character at once suggests that these were in no case *wholly* due to either the alternate storing and unloading of water in the form of ice on the lands or, as Suess and Schuchert have it, to retreats occasioned by periodic deformation and deepening of oceanic basins and ensuing slow submergence by deposition of land detritus in the seas. Doubtless both of these processes contributed to the displacements of the strandline—clastic deposition continuously, and deglaciation more occasionally, in effecting submergence; accumulation of glacial ice and submarine deformation in effecting emergence. In all cases the work of these agents tended to produce an even rise or fall of the sea level. So far then as the coastlands are concerned the displacement of the strandline by these two causes would have been essentially eustatic.

But we know that, commonly at least, the displacement of the strandline was not entirely eustatic but more or less differential even in short distances. Other causes, such as deformation by loading, variable gravitational attraction, etc., must have contributed to produce the complex result. Of these other factors, I am sure locally varying movements within the land masses themselves, including the more or less submerged shelf, are the most important. What the relative effects of the several factors in each particular case may have been constitutes a most difficult and varying problem. These proportions can not possibly have been the same in all cases. Besides only one of the causes of submergence—namely, the filling of the sea basins with deposit—could have been constantly in operation though obviously most variable in the volume of result. Then, on the other hand, either sudden or gradual deepening of an ocean basin would by itself suffice in effecting emergence.

Up to a certain point I agree with the suggestions of Penck, Daly, and others concerning the competence of the Pleistocene

ice sheets to effect considerable lowering of sea level; and the evidence indicating warping of the land surface, because of the uneven distribution of the ice load, as first pointed out by Jamieson, seems to me reasonably compelling. I believe also that in deglaciation the land surface largely re-established itself by elastic, or rather, isostatic rebound to preceding relief.

Though accepting in modified form the idea of glacial control of particularly Pleistocene sea levels, it is not to be denied that the present well-known occurrence in Newfoundland and in remote outlying stations along the coast of New England and the Maritime Provinces of many plants characteristic of the Coastal Plain of New Jersey and the south tends, as expressed by Barrell,[2] "to rule out the hypothesis that emergence was controlled only by the level of the ocean water as controlled in turn by glaciation." The extraordinary distribution of plants referred to could not be brought about by natural processes today. Evidently the northern occurrence of this flora is to be viewed as remnants of a preceding continuous distribution established when the climate of the northeastern coast was warmer and its coastal strip higher, wider and much less broken by water gaps. These required land conditions may be readily conceived as having obtained during, and as having resulted from, the ice loading of the glaciated regions to the west and northwest. As the latter sank under their growing load the continental shelf bulged its surface above sea level. But whether the plant migration could have been effected during the maximum extent of the Labrador Pleistocene ice sheet is so doubtful that Barrell[3] thought it necessary to assume delay in the settling back of the upwarped marginal zone after the removal of the ice sheet. As defined by Barrell, his hypothesis is "that the weight of the ice sheets caused crustal depression directly below the load, but moderate elevation in a wide zone beyond the load. Upon the removal of the ice it appears the first isostatic upwarping carried up higher this marginal upwarped zone with it. Being already an upswollen tract the broader regional movement car-

[2] Amer. Journ. Sci. 40: 17. 1915.
[3] Idem. pp. 19–21.

ried it up to a level where it became unstable and a slow settling back occurred as an after-effect, coincident with the last stages of upwarping over the centers of glacial load. The actual evidence at hand does not decide between these hypotheses. The association with the close of glaciation appears to favor a genetic connection with deglaciation, but, on the other hand, it remains to be demonstrated why the extra-marginal zone should rise together with the region directly glaciated, or that the cycle was restricted to such an extra-marginal zone."

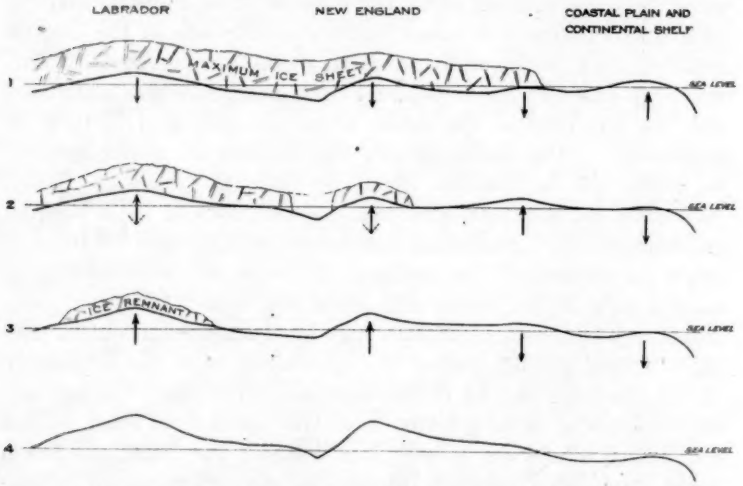

FIG. 1.—Generalized profiles of eastern North America in Pleistocene stages, indicating isostatic vertical movements of surface of lithosphere in process of deglaciation: 1, during maximum extent of ice sheet, when the outer part of continental shelf was emerged; 2, when the ice load had retreated from the present coastal strip; 3, a later stage when the ice sheet had been reduced to the area of Labrador; 4, present relief of land, with submergence of continental shelf. Approximately similar conditions may be supposed to have obtained in the growing stages of the ice sheet.

That the eastern margin of the continent, south of Labrador, did rise to higher levels than the present during the retreat of at least the last Pleistocene ice sheet seems, with Barrell's interpretation of Woodworth's[4] data and conclusions regarding

[4] N. Y. State Education Department, Bull. 84: 1905.

"Ancient Water Levels of the Champlain and Hudson valleys," highly probable. Indeed, supported as this evidence is by the facts concerning the distribution of the Coastal Plain flora just alluded to, emergence of this marginal area as this time may justly be accepted as reasonably established. As will have been observed in the quotation, Barrell's hesitancy in adopting this hypothesis arose mainly from the uncompleted demonstration of "why the extramarginal zone should rise together with the region directly glaciated."

In thinking this matter over the possible solution of the difficulty somewhat crudely illustrated in figure 1 has been reached. The diagram represents in generalized profile four Pleistocene stages of eastern North America, the profile running southeastwardly from Labrador to the edge of the continental shelf. The stages are represented separately, showing relief of land surface in each and the extent of the ice sheet in the maximum and two partly deglaciated stages. The fourth represents the present condition. One of the new features is that as the ice retreated the normally positive strip bordering the present eastern shore responded at once to the release from directly applied weight pressure by rising. Emergence of this Piedmont and Coastal Plain strip would be further insured by the necessity of maintaining isostatic balance with the outer strip of the continental shelf which had bulged to emergent status by subterranean flow from beneath the ice loaded land. In consequence, as the ice sheet retreated the emerged outer part of the continental shelf began to sink, whereas the strip along the landward side of the present shore rose. Among the physiographic changes that may be supposed to have occurred at the time of this southwardly decreasing elevation of the coast lands north of Baltimore is the cutting of the now buried deep channel of the lower Hudson; also the sharp southward deflection of the Delaware and Susquehanna rivers. During the preceding maximum extent of the ice sheet Maryland is supposed to have stood higher than at present and the lower stretches of these rivers either flowed northeastwardly or they emptied more directly and much sooner into the sea, which then probably covered the New Jersey part

of the Coastal Plain and extended widely into the eastern valleys of the adjacent Appalachian region. As the ice sheet retreated Maryland settled back while the coast lands to the north rose. The resulting emergence and the reversal of the tilt of the land surface must have produced corresponding changes in the direction of flow of affected rivers. Obviously results like these required practically immediate isostatic response to both the accumulation and the removal of the burden of ice and not as Barrell thought, "a deferred intermittent, and possibly oscillatory, readjustment." (Op. cit. p. 21.) On further retreat of the ice front the upward movement of the latter was arrested and finally reversed, so that it shared in the general subsidence of the marginal area when the complete withdrawal of the ice sheet permitted isostatic rebound of the unloaded interior highlands to their preceding and present normal land altitudes.

In consequence of the bulging of the sea bottom adjacent to shore lines that in the maximum spread of the ice sheets had sunk beneath the load of ice, the capacity of the ocean basin must have been correspondingly lessened. This in turn would have tended to retard and finally reverse the downward direction of the change in sea level previously prevailing on account of subtraction of ocean water for the making of the ice sheet. That is, it would have caused actual raising of sea level except in those parts of the shore line that were covered by the ice sheet and therefore directly affected by its weight. The upward movement of the sea level thereby occasioned would have been worldwide and eustatic.

But the displacements of the Pleistocene strandline along the Atlantic Coast that were in any wise connected with glaciation must, because of varying conditions arising from the fact that the ice sheets did not reach the shore line south of New Jersey, have varied greatly in amount and direction at different places. It was only in the early stages of glaciation, before peripheral elevation of the surface of the lithosphere with respect to areas bearing ice loads had progressed to the stage wherein it caused material lessening of capacity of ocean basins, that the sinking

of sea level could have been eustatic. On the reversal of this sea level movement, when the Pleistocene ice sheet stretched to the shore and when as stated above, the consequent bulging of adjacent parts of the continental shelf reduced the capacity of the ocean basin, the change in sea level as manifested in the advance and retreat of the Atlantic shore north of, say Cape Hatteras, was far from eustatic. During this maximum extent of the Labrador ice sheet, the ice-covered near-shore lands about the Gulf of St. Lawrence must have sustained extensive submergence. Southwardly from northern Maine to New Jersey the amount of this submergence decreased perhaps to its minimum. On the other hand, in Maryland, which I take to have lain at that time within the belt of peripheral isostatic elevation, the land was pushed up with resultant apparent or relative sinking of sea level. Farther south, beyond the belt of peripheral bulging, the Atlantic shore probably shared in the eustatic rise of sea level that prevailed generally because of the temporarily decreased capacity of ocean basins except in the areas affected immediately and differentially by the ice sheets.

Correlation of Pleistocene sea beaches in Maryland and Maine therefore suggests and perhaps requires comparison of the high beaches in Maryland with low beaches in New England.

Because of this dissimilarity in manifestation, it seems to me that it is only in the warm temperate and tropical zones lying well beyond the areas in which isostatic balance would be materially disturbed by known ice loading of lands, that the sequence and amount of the several glacially controlled Pleistocene changes of sea level are recorded in their proper relations to the actual fluctuations of the volume of sea water and to the capacity variations of the basins holding it. But even in tropical areas the complete sequence of the oscillations and the immediate cause of each cannot be worked out without taking strict account of what was happening at the same times in higher latitudes.

In thinking of the progressive and regressive sequences of movements it is well to remember that ice loading and sediment (rock) loading of epicontinental areas are comparable in their

deformational effects on the lithosphere only in one respect—that is, in both cases the loaded area sinks. They differ, primarily, in that the ice cap originates on, and spreads outwardly from, normally positive areas whereas the rock sediments are laid only in areas of relatively negative tendencies. Subsidence because of ice loading, therefore, is an abnormal process in that it is carried on under unusual conditions, so that normal gravitational tendencies are reversed; in the other case not only the process but the results also are perfectly in accord with the normal gravitational tendencies of the affected areas. Next, they differ in that the ice sheets presently melt away, whereas the water-laid rock deposits commonly remain as a permanent asset of the area covered by them. A third difference is that in the first cases the removal of the ice load tends to re-establish the normally positive tendencies of the deglaciated areas, whereas in the areas loaded with rock deposits their normal negative tendency is not reversed.

Finally, there is the rather generally accepted belief among stratigraphers and students of paleogeography that in the past the advances of the sea usually were slow and gradual, whereas the retreats were more rapid and relatively impulsive. Many facts in Paleozoic stratigraphy are cited in my *Revision* in support of this belief, and Barrell, in 1915, expressed himself as favoring the view.

Now, if we accept this conclusion it certainly does not help the hypothesis of measurable sea level fall by storing of oceanic waters in continental ice sheets. Obviously, the subtraction of water from the seas to make the ice sheets must have been a slow and on the whole gradual process; and the time consumed in the growth of the ice sheets probably was not materially shorter or longer than that required in their melting.

From these considerations it is clearly evident how exceedingly difficult is the proper determination of the part actually played by glaciation and ensuing deglaciation in the emergence and submergence of the continental borders. The fall and rise of sea level directly resulting from the storing of oceanic water to make a great ice sheet that later is returned to the sea is so

intricately connected and interwoven with genetically similar but at times oppositely directed general and local deformations of land areas and also of sea bottom areas adjacent to the strandline, that the reliable valuation of the two or more factors seems as yet practically hopeless. Moreover, it appears to me that only the early and the late stages of a period of glacial control could have made and left anything approaching world-wide and vertically equal records of consequent displacements of the strandline. The early stages would be those in which the lateral growth of the ice sheet had not yet reached the zone in which the weight of the ice would have caused extramarginal bulging and apparent lowering of sea level far in excess of the fall actually occasioned by transferal of water from the sea to the land. Similarly the later stages would be those following the retreat of the ice sheet to the same relatively innocuous limits.

It follows, then, that only the eustatic smaller shiftings of the Pleistocene sea levels may be definitely ascribed to storing and subsequent release of frozen water on the land. And for these even it is mainly their occurrence in a known ice age that induces one to admit their probable glacial origin. However, the larger and in most instances also much more local Pleistocene oscillations of the strandline, even granting that their causation is intimately connected with ice loading and unloading of land areas, belong to another category. Strictly speaking, these larger displacements have resulted from truly diastrophic causes and processes that are concerned with the maintenance of the isostatic equilibrium of the lithosphere.

Under the circumstances, then, I must agree with Barrell in concluding that the amount of water taken from the seas for the formation of the ice sheets was not a direct "major factor in the control of Pleistocene sea levels." Movements, acting within, beneath, and upon the lithosphere thus appear to have been the more effective factors.

That the marginal areas of the continents were at times elevated and folded is, of course, accepted by all—even by Suess and his followers, who speak of the continents as having the character of "horsts" and of the ocean basins as being perma-

nently "sunken areas." Suess, however, believed that the median areas of the continents are essentially stable, a view adopted by Schuchert, who holds "that the continent (North America) is a horst, that the great medial region remained unmoved, while the margins were often folded and elevated. The seas periodically flowed over this medial land—in fact, were elevated over it—owing to the detrital materials unloaded into the oceanic areas, thus filling them and causing them to spill over on to the lands."

I can not subscribe to this opinion. On the contrary, though accepting the idea of permanent oceans and continents, it seems to me that the crust of the lithosphere was subject to periodic movement away from the poles; that the surface of the lands was exceedingly unstable in the median areas as well as along the borders of the continents. Schuchert's paleogeographic maps, indeed, offer convincing proof of such instability; and the more detailed maps made since his appeared, further substantiate my claim.

In reaching these conclusions I am mainly influenced by a lifetime study of Paleozoic formations and their faunas. The criteria and principles used in the course of these stratigraphic investigations are defined and discussed in my *Revision of the Paleozoic Systems* published in 1911. In this work more than 100 previously undescribed instances of differential vertical movements of lands and consequent shifting of seas are discussed in varying detail. Since 1910 much additional information has been gathered concerning such oscillations in North America.

On this occasion I shall mention briefly some of the more convincing of the published cases and in greater detail a few of the more recently determined instances—enough of both to show that from the beginning of Cambrian time the surface of the continents was exceedingly unstable and subject to frequent oscillation, and that the epicontinental seas were correspondingly inconstant, shallow, relatively small and frequently withdrawn in part or entirely. Even in the same geological provinces the outlines of the new sea may agree essentially and

often very closely, in parts, with the next preceding or some earlier sea, but in other parts the new shoreline departs radically from the older.

These movements occurred in Paleozoic ages which, unlike the Pleistocene, have left no record of great ice accumulations. Doubtless even in the Paleozoic there were times of relative frigidity—when some of the higher parts of the marginal lands were ice-covered, in some instances attaining locally to glacial conditions. Here and there regular tillites are indicated, notably, as recently brought out by Dr. Edwin Kirk, in the Silurian deposits along the coast of Alaska. Occasionally, too, transportation of bulky erratics by heavy shore ice is suggested, as for instance by the late Ordovician Rysedorph hill conglomerate near Albany, N. Y., and the great masses of unworn limestone of Ordovician and Silurian ages found in the early Pennsylvanian Caney shale of eastern Oklahoma. But the Paleozoic history of North America so far as known affords no suggestion of icy ages comparable to the Pleistocene period in the amount of water abstracted for the formation of the ice sheets. Moreover, by far the majority of the displacements of the strandline in the continental seas occurred at times and places that give no indication whatever of particularly cool climates. On the contrary, the entombed faunas in the overlapping and interfingering marine formations could hardly have lived in the shallow seas if the climate of the adjacent lands had not been mild.

With the data in hand I feel warranted in asserting that the level of the Paleozoic continental seas was seldom appreciably affected and certainly never controlled by glaciation. Besides, the apparently irregular, though doubtless rhythmic, shiftings of the strandline almost without exception indicate local differential movement in the continental surface. And these movements must have been connected with other more general movements, requiring at times partial or complete withdrawal of the waters from the land depressions, at other times permitting readvance in the same or some other newly depressed land basin.

The varying distribution of marine deposits of successive ages naturally suggests differential upward and downward move-

ment of the lands as the immediate cause. If the submergences had been occasioned solely by rise of the waters, the successive submergences would have been always similar in geographic pattern and different only in lateral extent. In fact, a general similarity or repetition of old patterns is recognizable, but there is also exceeding diversity of expression; and often the difference is greatest when directly succeeding stages are compared. Often again, when one stage appears to have been very different from the next, the following third or fourth may be very much like the first. Only oscillatory movements or warping of the land surfaces could produce such results. The area affected by such movements may be very large, as, for instance, during the middle Ordovician and middle Silurian, when nearly half of the continent of North America was involved. During these periods the Gulf waters seem at certain times to have been completely withdrawn from the southern part of the continent, the middle and northern parts at such times being tilted so that the boreal sea extended southward beyond Chicago and occasionally as far as northern Tennessee.

Strictly, these widely operating movements hardly fall under the category of epeirogenic movements. On the other hand, they are not truly orogenic, if that term is to be confined to movements originating in shrinkage of the centrosphere. Apparently they indicate a combination of causes, perhaps beginning or ending with the play of orogenic factors that built mountains in the submarginal areas whereas the warping and deformation of the more stable interior areas was mainly occasioned by the necessity of isostatic readjustments to stresses incident to the greater deformations of the orogenic movements.

Then there were many relatively local changes in the strandline of continental seas that may be explained only by assuming correspondingly local differential, vertical movements of the lithosphere. I do not refer to movements connected with volcanism. On the contrary, the best examples of the kind in mind are found in areas but rarely or not at all directly affected by volcanism. These differential movements indicate actual elevation of one area while another nearby was sinking. More-

over, in the next recorded age the directions of ensuing movements at the two places often were reversed. The phenomenon might be likened to a gently convex platform supported in the middle and tilted alternately to the east and west and at other times to the north and south. The condition is recognized by the alternate presence and absence of sediments of particular ages on opposite sides of the tilting platform. (See figure 2.)

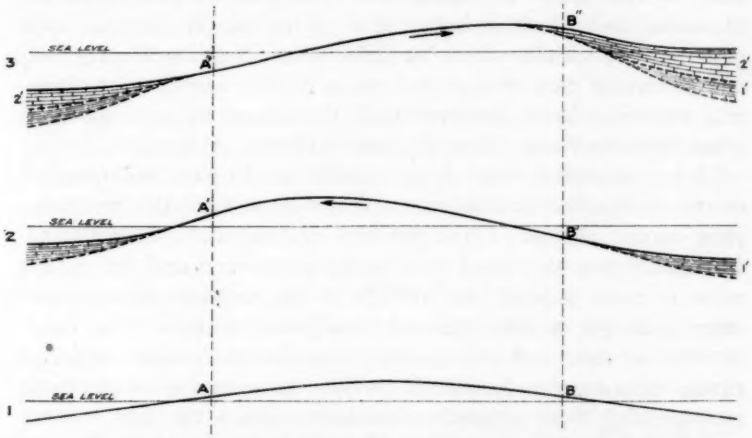

FIG. 2.—Diagram illustrating tilting of interior areas of uplift (for example, the Cincinnati dome), and the consequent variations in amounts of advance and retreat of the sea on their opposite sides. Arrows indicate direction of horizontal stresses. The letters A, A′, A″, on the one side and B, B′ and B″, on the other, mark the same points on the flanks of the dome in all of the three stages. In 1 the sea laps equally on both sides; in 2 the elevation of the dome is accentuated and its summit has migrated to the left, while the sea has advanced much more on the right side than on the left; in 3 the summit has migrated in the opposite direction so that the deposits of the preceding stage on the right flank are largely emerged whereas on the submerged left flank the new sea widely overlaps the deposits of the two preceding stages (1′ and 2′).

Comparative studies of the Paleozoic deposits in the Appalachian Valley region, from eastern Pennsylvania on the north and central Alabama on the south, have brought out over a hundred clearly defined examples of such oscillations. They are manifested by the restricted distribution or local deposition of many overlapping formations having maximum thicknesses of from 200 to over 2,000 feet. In many cases these forma-

tions are wholly or mainly confined to one or more narrow, trough-like, longitudinal divisions of the Appalachian geosyncline and commonly to one or another of three divisions of the geosyncline that are more or less effectively separated from each other by low transverse axes. The most northerly of these broad axes passes across the valley between Carlisle and Lebanon, Pennsylvania. It is known as the Harrisburg axis. The next to the south intersects the valley of Virginia between Staunton and Harrisonburg. The third or Wytheville axis passes across southwestern Virginia, which is today the highest and narrowest part of the great valley. The fourth axis crosses in a more northerly direction than the others through the belt lying between Rome, Georgia, and Gadsden, Alabama.

These transverse axes do not cross the longitudinal troughs of the geosyncline in continuous direct lines. On the contrary, their course zigzags within the varying limits of a broad band so that the northern head of a bay in one trough may extend 50 miles or more beyond the latitude of the southern head of another, younger or older, bay in an adjacent trough. The band is wide enough and was always low enough so that regional tilting occasionally permitted overlap of edges of formations transgressing from opposite directions. Often the axis formed an efficient barrier in one trough and was much less effective in the one next to the west or east. More rarely, a bay, terminated at the north by a transverse axis, connected laterally with waters in an adjoining trough in which the submergence was not stopped by the axis. Finally, at other times the axis offered no serious obstacle to the passage of the marine invasion. Of course, the individual troughs were submerged over and over again, but in none do we find representatives of all of the formations known to have been deposited in the Appalachian Valley.

Varying geographic expressions like these could have been made possible only by differential vertical movements in the concerned parts of the lithosphere, and these Appalachian oscillations in sea level were by no means small affairs. Most of them are measured by hundreds of feet and some by thousands.

Excellent and very interesting oscillations occurred about

those more inland and very ancient positive areas known as the Cincinnati and Nashville domes, the Ozark and Adirondack uplifts, and the Wisconsin peninsula. Of the many formations that are found on their flanks and which failed to pass over them much the greater number are confined to one or the other side. The sequence of formations on either side therefore differs greatly from that on the opposite side.

Much space is devoted in my *Revision of the Paleozoic Systems* to a description of the inequalities in areal distribution of the formations that were laid down on the flanks of these epicontinental domes. With a few corrections and modifications, in every case tending to emphasize rather than to weaken the argument based on the observed phenomena, the published statements concerning them in that work have been further substantiated by more recent investigations. Instead of overstating the number of oscillations in that paper we can now prove many more instances than were known or even suspected by me in 1910.

In New York State alone, the joint investigations carried on in the Ordovician shales and limestones on the south and west sides of the Adirondack mass by Doctor Ruedemann and myself, and on the Medina and Clinton formations with Mr. Hartnagle have increased the established cases of sea shifting implying more or less decided differential vertical movements in the adjacent land masses to more than twice the number contemplated when I wrote the *Revision*.

Similarly, the work of Mr. Charles Butts and myself, on the Mississippian formations in Illinois, Kentucky, Tennessee, and Alabama has developed oscillations of like character that were scarcely suspected six years ago.

Very notable additions to our knowledge of Cambrian and Ozarkian oscillations also have been made in the course of my work on the Paleozoic formations in Wisconsin. Before closing permit me to give some details concerning at least one of many similar new discoveries in this and adjoining States.

Only a few years ago the stratigraphy of the Cambrian deposits in the upper Mississippi valley was practically unknown

or at best only very imperfectly understood. Because of certain misapprehensions, now clearly understood, the correlations of the several sections by the State geologists of Wisconsin, Minnesota, and Iowa were not only inadequate but quite in error.

So long as the observed variations in character of deposits and their fossil faunas were supposed to indicate nothing more than merely local variations in contemporary seas and life it was almost impossible to work out the true relations of the beds in the largely drift-covered and hence discontinuous exposures of the Cambrian rocks. A new viewpoint was required; also closer investigation of bedding planes, greater accuracy in noting the vertical and geographic ranges of particular species and faunal associations and of particular beds. In short, it was necessary to employ more modern criteria, principles, and methods than had been used before.

When the work of revising the Paleozoic stratigraphy of Wisconsin was begun in 1914, the task seemed relatively simple in view of the success that had attended our investigations in the supposedly more difficult fields in the Appalachian region, about the Cincinnati and Nashville domes, and the Ozark and Adirondack uplifts. Indeed, the results of the first season's work in Wisconsin were so satisfactory to Doctor Walcott that he decided to publish my revised section in his work on the Dikelocephalid trilobites.[5] As therein given, the Upper Cambrian series in the Mississippi valley is divisible into six lithologically and faunally distinct formations, named from below upwards: the Mt. Simon sandstone, which rests on pre-Cambrian crystallines, followed in turn by the Eau Claire shale, the Dresbach sandstone, the Franconia (glauconite bearing) sandstone, the St. Lawrence formation of limestone, shale and sandstone, and the Jordan sandstone. Above these came the Lower Ozarkian Mendota limestone and the Madison sandstone, the last of which is overlain by the Oneota dolomite of the "Lower Magnesian" series. Aside from the determination

[5] *Dikelocephalus* and other genera of the Dikelocephalinae. Smith. Misc. Coll. 57: 1914.

of the lithologic and faunal sequence of the Cambrian in the western half of the State, the most important improvement brought about by the first season's work was the proof that the Mendota limestone and Madison sandstone are really post-Cambrian formations and not, as had been supposed previously, the eastern representatives of, respectively, the St. Lawrence limestone and the Jordan sandstone of Minnesota. In fact, it was then believed and has since been definitely proved that whereas the St. Lawrence extends uninterruptedly from Minnesota and Iowa across the southern half of Wisconsin and under cover of later formations into northern Illinois, the Mendota limestone is entirely absent to the west of a narrow trough running southeastwardly from the southern slope of the pre-Cambrain Baraboo quartzite range.

In the following field season of 1915 doubt arose as to the eastward extension of the Franconia formation to and beyond Madison. At this place there is a more or less decidedly calcareous sandstone formation, approximately 100 feet in thickness, which lies between unquestionable Dresbach sandstone and no less certainly established St. Lawrence limestone and shale. The intervening formation therefore seems to occupy the same stratigraphic position as the Franconia. But its lithological characteristics, except that it also contains considerable, though more disseminated glauconite, are quite different from those of the Franconia; and whereas good fossil remains of characteristic types are exceedingly abundant in the Franconia they appear to be much fewer and, so far as could be determined from the handful of fragments then procured, of different species.

In casting about for a means of determining the problem I thought of an old anticline that extends southwestward from the Baraboo range across southern Wisconsin into Illinois. This axis had previously been found to have had an important effect on the distribution of the Ordovician formations and it seemed worth while to see whether it had not been in existence, and functioning as a barrier, already in the Cambrian. Accordingly, a part of the season of 1916 was devoted to following the nearly

continuous exposures of Cambrian rocks in the bluffs and valley walls along Wisconsin River.

Beginning at Boscobel and going upstream, the Franconia, in typical development, was found to hold its own for a distance of about 20 miles, when it began slowly to lose thickness by overlap. The succeeding 15 miles, which brought us to the town of Lone Rock, sufficed to pinch the formation out entirely. Beyond Lone Rock, for a distance of about 10 miles, in which we passed through the town of Spring Green, the Franconia is absent, the top of the underlying Dresbach sandstone has risen considerably above the river level and is immediately followed by characteristically fossiliferous shales and limestone of St. Lawrence age. (See figure 3.)

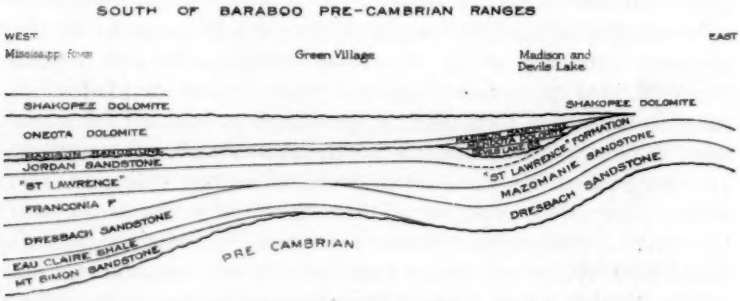

FIG. 3.—Section across southern Wisconsin, showing sequence of Upper Cambrian (St. Croixan) and Ozarkian formations, the apparently similar stratigraphic positions of the Franconia and Mazomanie formations, and the absence of both on the summit of the pre-Cambrian anticline.

Just east of Spring Green the closed contact between the Dresbach and St. Lawrence opens again to receive the wedge of magnesian sandstone whose age was the quest of the undertaking. Where first exposed in the bluffs east of Spring Green the Mazomanie sandstone, as the new formation is called, is about 10 feet thick. Four miles east of the town it has thickened to 80 feet, and at Fairy Bluff it reaches 100 feet. Wherever it rises to considerable heights above the valley bottoms in Dane, Sauk, and Columbia counties it forms cliffs, which is not at all true of the typical Franconia.

But, so far as positive evidence regarding the age relations of the Franconia and the Mazomanie is concerned, these investigations of the bluffs along Wisconsin River left the question as unsolved as before. Nor did we come any nearer to its satisfactory solution in the course of the following season's work when a series of sections was made on the south side and around the eastern end of the Baraboo range. But just before the close of the field studies in 1918 some very promising but under the circumstances inconclusive observations were made in sectioning the outliers and bluffs which dot the sandy plain of central Wisconsin. Namely, at one of these bluffs I found a perfectly characteristic Mazomanie cliff and beneath it a 2-foot

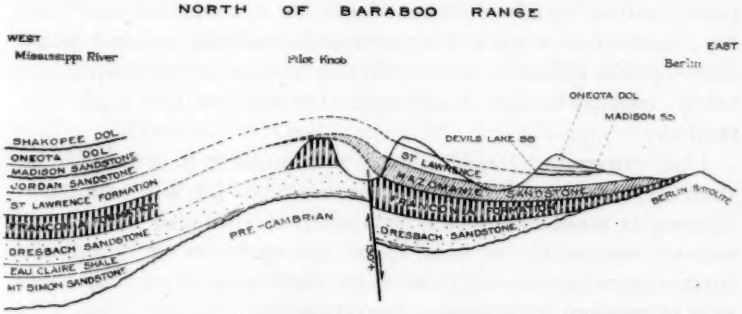

FIG. 4.—Section across central Wisconsin, showing greater eastward extent of the Franconia in this part of the State and intercalation of the Mazomanie between the top of the Franconia and the base of the St. Lawrence.

exposure of reddish sandstone that seemed to me to be of Franconia age.

However, the evidence at this place was not satisfactory to Doctor W. O. Hotchkiss, State Geologist, and Mr. F. Thwaites, who accompanied me on this as on most of the other trips through the State. Their doubts arose mainly from the fact that my interpretation required the assumption of a fault hitherto unsuspected between this bluff and Pilot Knob, which lies less than a mile to the northwest.

And so it was left to the work of the past summer to clear away all doubt, if possible. And it was cleared away. Other outliers in this vicinity were visited until finally we found two that were capped by Mazomanie and St. Lawrence and beneath the Mazomanie showed from 50 to 100 feet of profusely fossiliferous Franconia. Incidentally the presence of the fault just mentioned was unquestionably established. As an interesting and welcome confirmation of the earlier conviction that the Franconia is older than the Mazomanie—welcome despite the fact that it came to light after the case had been proved by actual superposition—I may add that two entirely new faunas, one from near the top, the other just above the base of the formation, were discovered in the Mazomanie. The upper of the two occurs rather widely distributed but in a sandstone so friable that it can not be picked up without crumbling in one's hand. Despite this difficulty a considerable collection was made and safely transported to Washington by soaking the sand with shellac.

I have described the solution of this problem in greater detail than may seem necessary, first because of its intrinsic value and interest as a new instance of oppositely overlapping formations, second because of its bearing on the question of differential surface movements, and third as an illustration of the thoroughness of modern stratigraphic investigations.

The case shows differential movement, first in the fact that the Franconia is confined to the western half of the State, whereas the preceding Dresbach was laid down on the east side and over the south side as well as the west. Next, the very different distribution of the Mazomanie shows reversal of the tilt from the west toward the east. Further—through the fact that the two formations are separated to the south of the Baraboo range by a broad strip, in which neither is present, whereas to the north of the pre-Cambrian range both formations were laid down so that the younger overlaps the older for a distance of at least 50 miles—it is proved that the movement was not simply an east-west reversal of tilt but that it was accompanied by additional local subsidence on the north where a depression was

formed that subsequently lodged a considerable embayment of the Mazomanie sea.

But this does not exhaust the known record of diastrophic movements of this time in Wisconsin. Uplifts of the relatively evenly distributed floor of Dresbach sandstone are indicated in many places; and depressions occurred in other localities so that the Franconia lapped over in such places on to the pre-Cambrian rocks. This occurs at Berlin and at Taylors Falls, towns located on opposite sides of the area covered by the formation. At Osceola, on the other hand, there is a narrow ridge on the surface of the Dresbach that completely cuts out the Franconia, though the formation is well developed both to the north and south of Osceola. Finally, we recognize two longer upwarps of the Dresbach floor that extend in a southwesterly direction from the central pre-Cambrian land mass which formed the backbone of the Wisconsin peninsula. These buried ridges divided the Franconia sea into basins sufficiently distinct to show well-marked differences in their respective depositional sequences and faunas.

But why pile up the evidence, the sameness of which must weary you. Suffice it to say that the phenomena indicating differential vertical displacements of the strandline are everywhere about us, and as abundant and well displayed in the areas of Paleozoic rocks as in those of more recent ages. One need but to compare a series of paleogeographic maps which, even despite their admittedly generalized and synthetic nature, yet show—unmistakably and clearly—variations in outlines of successive continental seas that would have been impossible if the land surfaces periodically invaded by them had not been subject to frequent oscillation and warping.

Physiographers, apparently, have paid little attention to these paleogeographic maps and the discussions of stratigraphic correlations that usually accompany them. Perhaps the reason for this oversight lies in the fact that most of them have been made by paleontologists—a kind of geologist who should be seen but not heard on physiographic and diastrophic questions. But, after all, does not the stratigraphical paleontologist deal with a

wider range of geological data and criteria than any other specialist in the science? Of them all, I regard the stratigraphical paleontologist the best equipped to bring out the dominant facts in questions of the kind before us. He has the same opportunities and desire to observe and note the phsyical factors of the problem, and in addition an appreciation of organic criteria that may not only be applied directly in the field but the tangible evidence—in the form of specimens usually small enough to be collected—may be carried to the laboratory and there be studied at leisure and as often as desired. I have found this of very great advantage.

For such reasons I would be disposed to prejudice in favor of earth students like Vaughan or Schuchert in cases of controversy with others who can not personally take into account and weigh the organic as well as the physical aspects of a problem. However, in the present instance, I have gathered so much competent evidence of my own that I feel warranted in reaching the conviction that the major factors in the control and migration of the strandline lie and have always lain in deformative movements within the lithosphere. These movements, whether large or small and whether due to shrinkage of the centrosphere, to local changes in crustal density, to unequal loading by rock or ice, or to erosion and further lightening of positive areas, are all primarily concerned with the maintenance of isostasy.

ABSTRACTS

Authors of scientific papers are requested to see that abstracts, preferably prepared and signed by themselves, are forwarded promptly to the editors. The abstracts should conform in length and general style to those appearing in this issue.

PHYSICAL CHEMISTRY.—*Application of the thermionic amplifier to conductivity measurements.* R. E. HALL and L. H. ADAMS. Journ. Amer. Chem. Soc. **41**: 1515–1525. Oct., 1919.

As a part of the general plan for the investigation of two component systems under pressure, a study is being made of the changes in solubility of a salt which occur when its aqueous solution, in contact with crystals of the solid, is subjected to pressure. In order to measure the changes of concentration which occur in the solution, while leaving it *in situ* in the pressure bomb, we note the changes in the conductance of the solution. For the attainment of sufficient accuracy in these measurements, the telephone which indicates the balance position of the Wheatstone bridge must be extremely sensitive since the allowable current through the network is limited by heating effects in the conductivity cell. The terminals of the bridge which ordinarily are connected to the telephone are joined to the "input" of a thermionic amplifier, of the type used in wireless telephony and telegraphy. The telephone is then connected into the output circuit of the amplifier. This arrangement has resulted in effectively increasing by approximately 50 times the sensitivity of a sensitive high-resistance telephone. Any ordinary telephone becomes a more sensitive instrument with the amplifier than the best telephones without it. R. E. H.

PHYSICAL CHEMISTRY.—*Equilibrium studies upon the Bucher process.* J. B. FERGUSON and P. D. V. MANNING. Journ. Ind. Eng. Chem. **11**: 946–950. Oct., 1919.

An investigation of the Bucher or cyanide process for the "fixation" of atmospheric nitrogen was undertaken at the suggestion of the War Department. Experiments were made using pure chemicals and mixtures of pure nitrogen and carbon monoxide in known proportions. Curves were obtained showing (1) the relation between the carbon monoxide content of the furnace gases and the yield of cyanide, and (2)

the relation between the carbon dioxide content of the furnace gases and the yield of cyanide, both at two temperatures. The curves indicate that under certain conditions producer-gas may be used in the process and that the dissociation of sodium carbonate is probably one of the controlling chemical reactions. J. B. F.

ANTHROPOLOGY.—*Prehistoric villages, castles, and towers of southwestern Colorado.* J. WALTER FEWKES. Bur. Amer. Ethnology, Bull. **70.** Pp. 79, figs. 18, pls. 33. 1919.

This work is intended to meet the demand for information regarding the prehistoric ruins in and about Mesa Verde National Park which was created for the preservation of the more important remains in this region. The Bureau of American Ethnology, in cooperation with the Department of the Interior, has done much by the excavation and repair of these monuments to increase their educational value, and in order to make known the nature of that work, the Bureau is publishing from time to time short papers on these monuments and their builders. Most of the structures here enumerated have long been known to cowboys and sheep men, and many have been described by archeologists, but this knowledge is local or scattered in many publications, often inaccessible to the general public. It is the purpose of the bulletin to collect this material in one publication and to show the relation of towers, castles, and other remains to the great cliff buildings of the Mesa Verde National Park. Particular attention is paid to the architecture of the ruins and the fact is pointed out that we have in buildings an index of the social organization of the people to whom they owe their origin. J. B. SWANTON.

OCEANOGRAPHY.—*Physical characteristics of the ocean depths.* G. W. LITTLEHALES. U. S. Naval Institute Proceedings, **6:** 45. Pp. 16, figs. 15. Jan., 1919.

The extent to which the ocean has been sounded and the conformation of its basins are described. The depth and bulk of the ocean, and the salinity, density, gases, temperature, pressure, compressibility, and viscosity of its waters are discussed. The penetration of light into the depths, oceanic movements and circulation, and marine deposits on the bottom are subjects also included. G. W. L.

SCIENTIFIC NOTES AND NEWS

RECENT ACCESSIONS AT THE NATIONAL MUSEUM

The National Museum has recently acquired, through exchange with the Carnegie Museum of Pittsburgh, a complete skeleton of the small extinct camel, *Stenomylus gracilis*, one of about forty skeletons found in Sioux County, Nebraska. *Stenomylus* was very abundant during the Miocene. It was about the size of a large sheep, with the slender, graceful limbs of an antelope.

A model restoration, of about one-twelfth natural size, of the American mastodon has been acquired by the Section of Vertebrate Paleontology. The original of the model was made by Mr. CHARLES R. KNIGHT, the animal sculptor.

240 specimens of Philippine Annelids, including types of several new species and comprising the remainder of the collection reported on by A. L. TREADWELL and RUTH HOAGLAND, has been received from the Bureau of Fisheries. Mr. JOHN B. HENDERSON has given a collection of 200 specimens of mollusks comprising 33 species of Sphaeridae and Naiades. Miss EMILY A. CLARK, of the Sudan Interior Mission, has presented a collection comprising 18 species of mollusks from southern Nigeria.

The Division of Birds has received 35 birds sent by Dr. W. L. ABBOTT from Santo Domingo; and 1,298 birds from the A. H. JENNINGS estate. The Jennings collection contains a skin of the extinct passenger pigeon.

The Mesa Verde collections of Dr. J. W. FEWKES are being classified by Mr. RALPH LINTON, of Harvard University. The Bureau of Ethnology has also received part of Mr. JEANCON'S collection of pueblo antiquities from the Chama district of New Mexico.

Dr. WALTER HOUGH has arranged a new exhibit showing the development of illuminating devices.

An exhibit has been installed in the Division of Medicine to show the types of balances used in weighing medicines and the progress which has been made in these balances. The first of the series is a single beam, double arm prescription balance made about 1840.

NOTES.

A pre-organization meeting of those interested in an American Meteorological Society was called by Dr. C. F. BROOKS at the Cosmos Club on December 20, following the meeting of the Philosophical Society. The Meteorological Society was formally organized at St. Louis during the meetings of the American Association for the Advancement of Science, with the following officers for 1920: *President*, R. DEC. WARD, of Harvard University; *Vice-President*, W. J. HUMPHREYS, of the Weather Bureau; *Treasurer*, R. E. HORTON; *Secretary*, CHARLES F. BROOKS, of the Weather Bureau. A coordinate meeting for presentation of papers was held in New York City on January 3, 1920.

Following the recommendation of the map-making conference,[1] a Board of Surveys and Maps has been created by executive order for the purpose of coordinating the activities of the map-making agencies of the Federal Government.

Life memberships in the National Geographic Society "in recognition of eminent services for the increase and diffusion of geographic knowledge" were awarded from the Jane M. Smith Life Membership Fund on December 22 to the following: FRANK G. CARPENTER, O. F. COOK, ROBERT F. GRIGGS, WILLIAM H. HOLMES, STEPHEN T. MATHER, E. W. NELSON, JOSEPH STRAUSS, and WALTER T. SWINGLE.

The magnetic-survey vessel *Carnegie* left Washington on October 9, on a two-year cruise of 64,000 nautical miles. She arrived at her first port of call, Dakar, Senegal, West Coast of Africa, on November 23, but on account of the bubonic plague sailed a few days later and is now enroute to Buenos Aires, Argentina, where she was expected to arrive about the end of January. The scientific personnel of the present cruise consists of the following: J. P. AULT, in command; H. F. JOHNSTON, magnetician, second in command; RUSSELL PEMBERTON, surgeon and observer; A. THOMSON, H. R. GRUMMANN and R. R. MILLS, observers.

Dr. EDSON S. BASTIN terminated his work as geologist in charge of the Division of Mineral Resources of the U. S. Geological Survey on December 26, 1919, to become Professor of Economic Geology at the University of Chicago. For the present he will retain his Survey connection, on the per diem roll.

Mr. E. F. BURCHARD, geologist in charge of the iron and steel section of the U. S. Geological Survey, has been granted a ten months' leave of absence and will make geologic investigations in the Philippines.

Mr. A. A. CHAMBERS, chemist in the Water Resources Branch of the U. S. Geological Survey, has resigned to accept a position with the Youngstown Sheet and Tube Company as chemist in their steel laboratory.

Mr. J. C. CRAWFORD resigned from the Bureau of Entomology on December 1, 1919, and has gone into the real estate business in Washington.

Mr. A. J. ELLIS has been appointed assistant chief of the Division of Ground Waters in the Geological Survey and will be acting chief of the division in the absence of Mr. MEINZER in Hawaii.

Mr. W. A. ENGLISH, geologist, has resigned from the U. S. Geological Survey to examine oil lands for the New Zealand government and also to make oil investigations in the Far East for New York interests.

Messrs. O. W. FERGUSON and P. M. TRUEBLOOD, of the Coast and Geodetic Survey, have completed a line of precise levels across New York State, from the northern end of Lake Champlain to Niagara Falls.

[1] See This JOURNAL, 9: 605. 1919.

Dr. J. A. FLEMING, chief of the magnetic survey division of the Department of Terrestrial Magnetism, Carnegie Institution, sailed for Buenos Aires on December 31 in order to meet the *Carnegie* there, and will return to Washington by way of the Department's observatory at Huancayo, Peru.

Mr. OWEN B. FRENCH, who has been professor of geodesy and practical astronomy in the Government Institute of Military Surveying, Peking, China, since April, 1918, returned to Washington in December, 1919, having completed the work he had under contract. Although requested to renew the contract, he preferred to return to the United States to resume his previous occupation as consulting geodesist.

Dr. E. C. HARDER, geologist of the U. S. Geological Survey, has resigned to become geologist for the Republic Mining and Manufacturing Company and associated companies, with offices at 1111 Harrison Building, Philadelphia, Pa.

Mr. K. C. HEALD is on leave of absence from the U. S. Geological Survey and is engaged in oil reconnaissance work in Colombia.

Dr. L. O. HOWARD, chief of the Bureau of Entomology, Department of Agriculture, was elected president of the American Association for the Advancement of Science at the St. Louis meeting in December. Dr. Howard has been Permanent Secretary of the Association since 1898. Prof. E. L. NICHOLS, of Cornell University, has been elected General Secretary of the Association under the new constitution, and the office of Permanent Secretary will probably be filled by the executive committee of the Council within a few months.

Dr. ALES HRDLICKA, of the Smithsonian Institution, left Washington early in January for the Far East, in the interest of his studies on the origin of the American Indian, and of the organization of anthropological research in China. He expects to return in May.

Prof. TAMIJI KAWAMURA, assistant professor of zoology in the Imperial University of Kyoto, Japan, visited Washington in December, giving particular attention to the work of the Division of Physical Anthropology and the Division of Marine Invertebrates of the National Museum.

Dr. BRADFORD KNAPP, director of extension work in the southern States for the Department of Agriculture, resigned on January 10, to become dean of the College of Agriculture, and director of the Experiment Station, University of Arkansas, Fayetteville, Arkansas.

Dr. ADOLPH KNOPF, geologist of the U. S. Geological Survey, has been appointed lecturer in geology at Yale University for the second term of the present academic year.

Mr. C. E. LESHER, geologist in charge of coal statistics in the U. S. Geological Survey, has resigned to accept a position as statistical expert with the National Coal Association.

Dr. G. F. LOUGHLIN has been appointed chief of the Mineral Resources Division of the U. S. Geological Survey, in the place of Dr. E. S. BASTIN, who becomes professor of economic geology at the University of Chicago.

Dr. M. W. LYON, JR., formerly professor of pathology and bacteriology, George Washington University, and at one time connected with the Division of Mammals, U. S. National Museum, and Captain in the Medical Corps during the war, has left Washington to take charge of pathological work for a group of physicians at South Bend, Indiana. His present address is 214 La Porte Avenue, South Bend.

Dr. G. P. MERRILL, of the National Museum, was elected vice-president of the Geological Society of America at the meeting at Boston, December 29–31, 1919.

Mr. JOHN MIRGUET, of the Division of Marine Invertebrates, National Museum, detailed to the U. S. Bureau of Fisheries' steamer *Albatross* to care for biological material secured, has returned to Washington after a trip along the southern Atlantic coast down to the Yucatan Channel.

Dr. CHASE PALMER, chemist of the U. S. Geological Survey, has resigned to accept a position as chief chemist in the fuel oil department of the Southern Pacific Company, at Bakersfield, California.

Rear Admiral JOHN ELLIOTT PILLSBURY, U. S. N. (Retired), president of the National Geographic Society, died on December 30, 1919, in his seventy-fourth year. He was born at Lowell, Massachusetts, December 15, 1846, and graduated from the United States Naval Academy in 1867. In addition to his varied service with the Navy, he spent one year at the Hydrographic Office and ten years with the Coast and Geodetic Survey. During this period he made extended investigations of the ocean currents off the south Atlantic coast of the United States.

Mr. S. A. ROHWER, of the Bureau of Entomology, has been appointed Honorary Custodian of Hymenoptera in the Division of Insects of the National Museum.

Mr. WILLIAM L. SCLATER, of the British Museum, visited Washington in December.

Dr. R. H. TRUE, of the Bureau of Plant Industry, Department of Agriculture, was elected secretary of Section G (Botany) of the American Association for the Advancement of Science, at the St. Louis meeting in December.